DOCUMENT RESUME

ED 103 398 SP 009 005

AUTHOR Asbury, Charles A.
TITLE Why Educational Research Is of Limited Use to the Community.
PUB DATE [75]
NOTE 7p.

EDRS PRICE MF-$0.76 HC-$1.58 PLUS POSTAGE
DESCRIPTORS Community; *Educational Research; *Relevance (Education); *Research Problems; *Research Utilization

ABSTRACT

Educational research is in drastic need of a new and different perspective. Some factors which cause this research to be of limited use to the community are: (a) a great deal of data is unusable because it is either wrong or too meagerly supported to judge its reliability; (b) there is a lack of communication among researchers resulting in a great deal of duplication of effort; (c) Krathwohl suggests that the impact of educational research is severely limited by the size of the research and development effort in proportion to the size of the field it is intended to affect; (d) research consumers are frequently ignorant of what the researcher is doing and distrustful of her or his efforts; (e) researchers have been too hasty in generalizing findings from the laboratory to classroom behavior; and (f) educational research often seems to have little practical application. It appears that educational research may have to settle for the role of information purveyor rather than that of change catalyst. (PB)

BEST COPY AVAILABLE

5.12

SP

ED103398

WHY EDUCATIONAL RESEARCH IS OF LIMITED USE TO THE COMMUNITY

U.S. DEPARTMENT OF HEALTH, EDUCATION & WELFARE
NATIONAL INSTITUTE OF EDUCATION
THIS DOCUMENT HAS BEEN REPRODUCED EXACTLY AS RECEIVED FROM THE PERSON OR ORGANIZATION ORIGINATING IT. POINTS OF VIEW OR OPINIONS STATED DO NOT NECESSARILY REPRESENT OFFICIAL NATIONAL INSTITUTE OF EDUCATION POSITION OR POLICY

Prepared by:
Dr. Charles A. Asbury,
Professor of Education
Howard University

SP009 005

WHY EDUCATIONAL RESEARCH IS OF LIMITED USE TO THE COMMUNITY

It should come as no surprise to anyone familiar with the subject, that the history of research in education is replete with dismal instances of irrelevance and confusion. A casual study of a journal series over an extended period of time frequently reveals that much (some would say "most") of the research that is done is ill-conceived, methodologically unsound, and of little consequence for the urgent business of improving the educative process. Especially appalling is the extent to which some investigations are intentionally esoteric and far removed from the mundane reality of educating real people for the pursuit of a quasi-adequate existence in the real world.

It is the thesis of this paper that educational research is in drastic need of a new and different perspective. At least a portion of this need may be attributed to the fact that the research endeavor, except in rare instances, has become so damnably expensive. The cost of doing a first-class experimental study is at times far in excess of any discernable benefits which might result from the conclusions. Still another influential factor is the recent demand for accountability in all facets of education receiving public support. A shocking revelation was recently offered by Boffey (1975) to the effect that approximately 50 percent of scientific data is unusable because it is either wrong or so meagerly supported that readers can't judge its reliability. While the article was primarily concerned with research in the natural sciences, it is probable that the phenomenon is accentuated in education due to the greater constraints placed on adequate controls.

Additionally, a sense of optimism prompts those who do not engage in educational research to hope that what researchers do will ultimately shed some light on the treacherous footpath to improved teaching and learning. Needless to say, the very nature of the problems selected for study at times reveals that this concern is not generally shared by those persons actively engaged in the research enterprise. It has become almost axiomatic in education that the so-called "better" journals will not even consider publishing a research report unless it is so far removed from ordinary educational matters as to be totally unfathomable to in-service workers in the field. Indeed, such ordinary people as classroom teachers and principals are supposed to accept as an article of faith the notion that mysterious titles which appear in such journals might help them solve some real problems if they could only be translated into a comprehensible form of communication. In fact, those of us who work in higher education are held in awe by many of our colleagues who work at other levels because of our seemingly divine ability to make journal articles intelligible, meaningful, and relevant.

The tragedy in all this is that it really isn't necessary. There is no defensible reason why educational research cannot be done at a level commensurate with productive theory construction and at the same time be communicated to consumers in a manner which avails itself of understanding and application.

An argument is sometimes advanced to the effect that research scholars should pursue knowledge for its own sake and should concern themselves primarily with the communication of their findings to other research scholars in a discipline. While this view pertains mostly to basic research it is nonetheless a fact that much of what gets published bears only a remote relationship to educational problems in the community. In order to get before the

public with his results, the educational researcher must first convince a journal editor and a review panel of referees that what he has done is sufficiently complex and obscure so as to be incomprehensible to the overwhelming mass of professional workers in the community.

This situation is further confounded by the fact that the majority of persons working in education at a given time are not trained to deal with the abstract elegance of many research reports. Graduate students, especially at the Master's level, are typically not exposed to enough instruction in statistics and/or research methodology to cope with the kind of stuff that finds its way into the journals. The expectation that one course in statistics and another in research methods can be supplemented by the busywork activity of abstracting articles to make one a competent research consumer is totally unrealistic. Consider also that in most instances at the undergraduate level, there may be even less formal exposure. This means that the person with only a bachelor's degree is not likely to seek periodical literature as a source of information or inspiration.

It is a truism that the ideas and motivations prompting the appearance of much of what gets published should have been surpressed before the author began the investigation. It is also true that the absence of a theory of education or instruction causes educational researchers to be eternally reinventing the wheel. One is amazed by the plethora of replications and duplications, if you will, which are undertaken in apparent ignorance of work which has already been done in a particular research area. Needless to say, this energy could be more constructively channeled into activities designed to focus what is already known onto the solution of some of our problems.

Educational research is also in need of some strategy designed to coordinate the efforts of researchers who are widely dispersed both geographically and theoretically. This strategy might very well emanate from the American Educational Research Association or from some agency of the Federal government such as N.I.E.. While diversity of points of view may be essential to the cross-fertilization of productive research pursuits, this does not have to mean that waste and duplication of effort are inherent by-products of this condition. A more expansive view suggests that educational researchers should be about the business of assessing the impact of their labors on the development of national policies and goals.

The Federal government assumes in its albeit inadequate support of educational research that it is mission-oriented. This means quite simply that practical consequences are anticipated that will contribute to meeting real individual, social, political, and technological ends. If such support is to continue, and hopefully, be increased, it is imperative that we begin a soul-searching examination of the educational system in the context of emerging local, regional, and national requirements. Educational researchers must seek to exert a more profound influence on public policies.Unless we can more effectively enjoin such vital issues as equality of educational opportunity, urban education, teacher militancy, educational relevance, and school control, we run the risk of becoming completely impotent.

Another view, suggested by Krathwohl (1974), is that the impact of educational research is severely limited by the size of the research and development effort in proportion to the size of the field it is intended to affect. Krathwohl asserts that with less than 1% of educational funds spent in research and development, the disseminating of those things which have already been developed is also severely impeded. Dissemination is perceived to

be a critical aspect of the overall process which has been almost totally short circuited. Still another difficulty is that some developments and innovations which do occur often give the impression of having been conceived without any apparent connection with the educational research establishment. This is exemplified in the influence of cognitive research on school science programs, the elaboration of Skinner's work with teaching machines and programmed learning, and the advent of competency based education programs Krathwohl (1974).

The situation is further complicated because research consumers are frequently ignorant of what the researcher is doing and are distrustful of his efforts. Brophy (1974) stresses again the point that this is largely due to the triviality of our research hypotheses and the irrelevance of much of what we do. In his view we are simply not studying problems that are related to the needs of the classroom teacher. The frustration and cynicism experienced by consumers will probably not be lessened until we begin to try to get information somebody wants, show its application to existing problems and/or program development, and then publicize and disseminate the overall process.

Even if educational and instructional objectives were stated concisely and explicitly and schools were achieving the objectives with a high degree of proficiency, it is conceivable that such an accomplishment might be irrelevant for the society as it exists. A highly efficient educational system achieving inappropriate objectives would represent real problems for any society. Discrepancies of this kind can only be discovered by a comparison of the performance of the system with an understanding of the economy, technology, politics, and values of the society as a whole. Thus it becomes important that educational researchers produce the kinds of investigations which will permit policymakers to evaluate the present relevance of the educational system to immediate and long-range needs and requirements.

It has also been suggested (Pell, 1975) that public support for educational research might be increased by establishing a strong lobby. At present there is a very real question in the minds of legislators as to whether educational research is really necessary. This is somewhat attributable to the incoherent, willy-nilly manner in which the outcomes of research and development are often presented. Educational researchers are apparently no more immune to "fadism" and parochial advocacy of pet conclusions than other segments of society.

Travers notes in the Second Handbook of Research on Teaching (1973) that the work is heavily loaded with emphasis on what is wrong with educational research and that the authors of the Second Handbook had greater difficulty in finding significant research to report than did the authors of the Original Handbook (1963).

Shulman (1970) has pointed to the differences between the human learning laboratory and the typical classroom. He also asserted that researchers have been too hasty to generalize findings even from the animal learning laboratory to classroom behavior. Referring to the existing gap between such studies and needed educational applications he then proposed an intermediate level of investigation to bridge the gap and create the basis for educational theory. Researchers should attempt to study behavior under the conditions that such behavior is expected to occur. To deal with the discontinuity between the settings of research and of educational application, Shulman (1970) proposed that a common language or set of terms for characterizing both experimental and educational settings is needed. It is strongly recommended that educational researchers begin to study environments on the basis of what

Shulman calls their "distinctive features". The marginal impact of past research and development has been attributed to the fact that because of low-scale funding, researchers have in the past been able to manipulate a relatively small number of variables, but seldom could attack whole problem situations.

Critical of research strategy in Psychology, Tukey (1969) observed that it continues to operate as if the individual Ph.D. thesis served as the prototype for scholarly efforts in the field. He recommended an increase in cooperative efforts in psychological research. The same thing might also be said of educational research. We must reorganize the structure of the research enterprise, abandoning the model of the individual entrepreneur and replacing it with the coordinated institutionally-supported research team (Shulman, 1970).

Recent clamor for accountability in education has created a need for educational researchers to assess the impact of what they do on the schools and the larger society. Most research, however, goes on without considering impact and ignoring evidence of no impact. Insufficient or inconclusive educational research does not much explain low impact. Although most research is both inadequate and ignored, an unsatisfactory piece of research may become influential (Clifford, 1973). Application of, or deference to research often depends less upon its quality or completeness than upon such social and ideological factors operating to form the zeitgeist of education and society at a given time (Clifford, 1973). In addition, more barriers to research implementation are owed to inadequate, incomplete professionalism than to the existence of a status secure "educational establishment". As far as progress is concerned, a very conservative public is often less responsive than either teachers or school administrators (Clifford, 1973).

In summary, it would appear that educational research may have to settle for the role of information purveyor rather than that of a catalyst for change. This is probably the case because education is so much intertwined with the political processes which permeate our national life.

A change in perspective is overdue which would have the effect of selling research in terms of the results expected of it, rather than in terms of the means of performing it. The solution of high priority problems has become a matter of national survival and educational research would be remiss if it did not seek to invoke a higher profile for influencing these developments than it has presented in the past.

REFERENCES

Boffey, Philip M. "Scientific Data : 50 percent Unusable", The Chronicle of Higher Education, 1975, Vol. 10, No. 1, 1 and 6.

Brophy, Jere E. "Some Good Five Cent Cigars", Educational Psychologist, 1974, Vol. 11, No. 1, 46-51.

Clifford, Geraldine T. "A History of the Impact of Research on Teaching" in Second Handbook of Research On Teaching. Edited by Robert M.W. Travers, Chicago: Rand McNally & Company, 1973.

Krathwohl, David R. "An Analysis of the Perceived Ineffectiveness of Educational Research and Some Recommendations", Educational Psychologist, 1974, Vol. 11, No. 2, 73-86.

National Center for Educational Research and Development. Educational Research and Development in the United States. Office of Education - U.S. Department of Health, Education, and Welfare, 1969.

Pell, Claiborne "Building Partnerships for Educational Research and Development", 1975, Vol. 4, No. 1, 11-12.

Shulman, Lee S. "Reconstruction of Educational Research", Review of Educational Research , 1970, Vol. 40, No. 3, 371-396.

Travers, Robert M.W. (Ed.) Second Handbook of Research on Teaching. Chicago: Rand McNally & Company, 1973.

Tukey, J.W. "Analyzing data: Sanctification or detective work?" American Psychologist, 1969, Vol. 24, 83-91.